twelve months I have been specially observing this matter both at St. Mary's Hospital and at the Hospital for Sick Children, Great Ormond-street, and have been surprised to find how common such cases are. It is certainly the rule, and from what I have found during this year I am inclined to think it will prove to be almost invariably true, that even in the mildest cases of this disease there is distinct cardiac dilatation demonstrable by percussion. My interest in this question was first aroused by observing the rapid dilatation which occurs in children the subjects of rheumatic pericarditis. I referred to this acute dilatation in a paper on the Treatment of Pericarditis in THE LANCET of July 22nd, 1893, and then attributed it to the weakening influence of the pericardial inflammation on the cardiac wall. But the discovery of the existence of similar dilatation in rheumatic cases free from pericarditis and the reflection that this dilatation does not occur in the pericarditis of Bright's disease or in suppurative pericarditis made me suspect that it must rather be a special result of the rheumatic poison, and I recalled Dr. Gaskell's researches on the action of acids on the heart of the frog. In the discussion on rheumatism at the British Medical Association meeting last August I suggested that this acute dilatation probably was the result either of an acid poison or of a toxin acting like an acid on the circulatory system. I now believe that this dilatation is not a rare phenomenon, but is present to a greater or less extent in all cases of true rheumatism. It may be detected also in many cases of chorea in which there is neither rub nor murmur, and this fact seems to be an additional link in the chain of evidence of the essentially rheumatic nature of chorea.

I am, Sirs, yours faithfully,

July 20th, 1896.

DAVID B. LEES.

ALBUMOSURIA.

To the Editors of THE LANCET.

SIRS,—I have read with much interest Dr. Lockhart Gillespie's article in THE LANCET of July 11th, and while in the main I agree with his conclusions I cannot help thinking that some of the details of his methods of testing for albumoses are faulty. On page 109, col. 2, Dr. Gillespie says: "One precaution should be taken, however, and that is not to add the acetic acid until the urine has been brought to the boil, and then at once to let it cool. In this way there is very little chance of the acid modifying the nature of the proteids present." Now, one knows that all proteids are modified by heat alone, and to obtain anything like accurate results all heat should be avoided in working with them. If either egg or serum-albumin be brought to the boiling point a certain amount is changed into some form of albumose. This point has more particularly been worked out by Cranstoun Charles. In the same column we further read: "Cold nitric acid gave no precipitate, showing that there was no albumin or hetero- and proto-albumose. Picric acid yielded a cloud disappearing on the application of heat—i.e., albumose or peptone. Saturation with ammonium sulphate caused a precipitate soluble in water. Saturation of the dissolved precipitate with sodium chloride caused no precipitate. The only proteid contained in the urine of this patient was, therefore, deuto-albumose and possibly some peptone, but of this last I am not certain." One can hardly understand why Dr. Gillespie, if he took all this trouble, did not extend his researches a little further and dialyse a little of the fresh urine through animal membrane and test the dialysate with dilute cupric sulphate solution and strong sodium hydrate solution. A decided pink would have revealed the presence of peptone. Solutions of *true* peptone are the only proteids which diffuse readily; albumin can hardly pass through. In fact, I find *true* peptone passes through in four hours or less; albumose takes many hours, say twenty-four (in summer one must not prolong dialysis because of the rapid changes which albumin undergoes). Dr. Gillespie, too, might have got more accurate results by employing the saturated solutions of sulphates in the entire analysis—namely, the sulphate of magnesium to throw out globulins, sodic sulphate to throw out albumin, and ammonium sulphate to throw out the higher proteids. By employing these methods I have mentioned—namely, dialysis and the use of the saturated solutions of sulphates—all error from heat can be avoided, and this is a very important point. One hopes Dr. Gillespie will continue his researches on this question of albumosuria, for as yet little is definitely known

of the subject, and we must feel grateful for what he has already done. Dr. Gillespie speaks of peptone. I suppose by this he means what we now know as true peptone (not the higher albumoses). A good deal has been published on peptone in the urine, but chiefly before accurate tests were known for peptone—when the pink biuret was considered accurate enough. It is now known that albumose gives the same reaction. Hence what has been called peptone may in reality have been albumose. I "hae ma doots" about *true* peptone being found in urine. Outside the body, at least, it is most difficult with the aid of pepsin or pancreatin to convert more than a small quantity of albumin into true peptone, the bulk of the solution being albumose.

I am, Sirs, yours faithfully.

Leeds, July 11th, 1896.

GORDON SHARP.

THE ETIOLOGY OF MALIGNANT GROWTHS.

To the Editors of THE LANCET.

SIRS,—Among the theses presented this year at the final examinations for the M.D. degree at the University of Pisa one deserves mention both for the importance of the subject and for the ability with which it has been treated. Signor Cassanello of Genoa, writing on the etiology of malignant growths, showed that both clinically and pathologically they may be referred to inflammatory processes of infective origin in like manner with tuberculosis, syphilis, and actinomycosis, but at the same time he acknowledged that a specific micro-organism capable of originating such growths has not yet been discovered, although this property has from time to time been claimed for many organisms belonging to the class either of schizomycetes, sporozoa, or blastomycetes. All attempts to produce carcinoma by inoculation of any of these germs have failed in the hands of even the best experimenters, and no specific organism of carcinoma has been discovered. It seems, however, to be proved that some blastomycetes, such as the *blastomyces niger* of Maffucci and Sirleo, are pathogenic and capable of producing inflammatory neoplasms, but these products have not been demonstrated to be of the same nature as malignant growths.

I am, Sirs, yours faithfully,

Pisa, July 13th, 1896.

P. SONSINO.

TREATMENT OF DIPHTHERIA BY ANTITOXIN.

To the Editors of THE LANCET.

SIRS,—The very frank admission of the leading medical journal of the world "that the results obtained in this country from the antitoxin treatment of diphtheria have been less striking than those upon the Continent" cannot but be satisfactory to one who has incurred no small measure of obloquy for the scepticism which his own observations of the treatment have, in spite of every predisposition to acceptance, forced upon him. According to your view the main contributory factor in the comparative failure which has hitherto attended treatment in this country—and it is with this country only, where we can check the statistics, that I pretend to deal—"is the simple fact of insufficient dosage."

To this I venture to reply that a considerable number of the early cures were those in which the dose administered was in minims fewer in number than is now advised in centigrammes. On the other hand, as I have shown in a recent article in the *Contemporary Review*, the mortality of cases treated in the hospitals of the Metropolitan Asylums Board in the late part of 1895 was twice as great as that in the earlier—in other words, in the period when the supposed advantage of larger doses was accepted and enforced.

Kassowitz, quoted by yourselves,¹ has remarked on the very high mortality from diphtheria in London in the last quarter of 1895. This increased death-rate can hardly be claimed to be due to a greater malignity of type, for the report of the Metropolitan Asylums Board not only fails to record such a circumstance, but expressly says that "the average severity of the cases has been about equal." My contention may be met by the rejoinder that only the severe cases were treated with the serum, but this is not so, for the report distinctly admits that in a considerable proportion of the cases no such classification was made, and that "in a

¹ THE LANCET, May 9th, 1896, p. 1319.

certain number of patients, being moribund at the time of their arrival and beyond the reach of any treatment, no antitoxin was given."

I am, Sirs, yours faithfully,
Mansfield-street, W., July 21st, 1896. LENNOX BROWNE.

THE HEALTH OF MARGATE.

To the Editors of THE LANCET.

SIRS,—It having come to my knowledge that a rumour has been circulated that several cases of typhoid fever have recently occurred in Margate I shall be obliged if you will kindly give me the opportunity to contradict it. The date of the last notification of this disease in this borough was over two months ago—viz., on May 15th, 1896. This, moreover, was an isolated case, no other having occurred about the same time, and it was immediately removed to our infectious diseases hospital, which is situated about two and a half miles out of the town.

I am, Sirs, yours very truly,
Margate, July 21st, 1896. A. W. SCATLIFE,
Medical Officer of Health.

THE INVERNESS MEDICAL SOCIETY.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of July 18th your Inverness correspondent, reporting the proceedings of the Inverness Medical Society, has the following: "The club question came up for discussion at several meetings and the members were unanimous in refusing to undertake club work unless the scale of rates were in accordance with that suggested by the society. The clubs did not see their way to agree to this arrangement and the medical officers resigned. The clubs in town joined together and advertised for a medical officer to undertake the work at their terms. Unfortunately for the credit of the profession a medical gentleman, Dr. Callender, formerly of Leeds, accepted the position, thus frustrating the efforts of the society in their endeavour to place the relations of the medical practitioners in Inverness and the clubs on a better financial basis than they had been hitherto." These sentences, in so far as they relate to the united clubs in Inverness and their medical officer, are false. The united clubs gave up their old terms and adopted *simpliciter* the scale of rates suggested by the medical society. The clubs unanimously elected Dr. Callender as their medical officer, and he holds the appointment at the scale of rates suggested by the medical society. Dr. Moir, secretary to the Inverness Medical Society, knew these facts six months ago. "Unfortunately for the credit of the medical profession" your Inverness correspondent has given a statement to you which (at any time during the last six months he had ample opportunity to find out) is false.

We are, Sirs, yours truly,
J. CALLENDER,
Medical Officer;
JAMES CORNET,
Hon. Sec. Committee of Combined Clubs.
Inverness, July 20th, 1896.

"A UNIQUE EXPERIENCE IN ETHER ADMINISTRATION."

To the Editors of THE LANCET.

SIRS,—I was interested in reading the report of this case in THE LANCET of July 11th, as I have been noticing for the last two years how little ether need be used to keep a patient fully anaesthetised. In an article on ether administration which appeared in the *Bristol Medico-Chirurgical Journal* of September, 1895, I stated that nearly all patients when once fully under with ether could be kept so with the indicator of Clover's apparatus midway between "0" and "1." I never find it necessary to keep the indicator at "2," as some books recommend, and I frequently succeed in maintaining complete anaesthesia for more than half an hour at a time with the indicator at "0." I quite agree with Dr. Carter that such cases show "how much more ether is frequently given than is necessary."

I am, Sirs, yours truly,
J. FREEMAN, F.R.C.S. Ed.,
Anaesthetist to the Bristol General Hospital.
Bristol, July 19th, 1896.

"THE REMUNERATION OF MEDICAL MEN."

To the Editors of THE LANCET.

SIRS,—The inadequate remuneration of medical men depends, I think, on two sets of causes: (1) those acting from without and which in the present state of things seem incapable of amelioration; and (2) those acting from within, from the conduct of medical men themselves. Among the first set of causes of ill-remuneration may be mentioned overcrowding of the profession, the multitudinous array of pestilent quacks, the ignoble army of patent medicine vendors with their omnipotent wares, and the counter-prescribing druggists mentioned by "H. C. L." in THE LANCET of July 4th. The district in which I live has lately been infested by quack cataract-without-operation curers, magic belt bouncers, and the ubiquitous Æsculapii of the Sequah type. With regard to patent medicines, bold advertisement and the fact of a Government stamp being affixed to each bottle or box account for their huge sale. The public imagine that what is stamped in this way must be genuine. It is astonishing how widespread the use of patent medicines is amongst all classes of the community. There is hardly a household in Great Britain which does not possess a box of magic pills or some other equally beneficent remedy. In the most well-to-do houses I have seen the well-known electricities of a certain deceased nobleman. An ounce of these costs 3s. 9d.—an exorbitant price for pure water. Nothing can put a stop to this sort of thing except the refusal of newspapers to publish quack advertisements, but these halcyon days can hardly be looked for. Counter-prescribing is a serious evil and should be put down with a strong hand, and the most serious aspect of it is that druggists in many cases prescribe without seeing the "patient" at all. A friend or relative (generally a female) comes to the shop, details the symptoms to the best of her ability, clutches the remedy, and marches off. Can anything be more dangerous and idiotic? Druggists say that medical men invade their province by making up their own medicines, but they seem to ignore the fact that there is a vast difference between themselves and medical men in the matter of clinical knowledge and the faculty of diagnosing disease. When a medical man makes up his medicine he does so with an intelligent and scientific object, which is more than can be said for the druggist, who is simply an empiric, as his medical knowledge is *nil*. Another favourite device of the ordinary druggist is to make up and advertise stock remedies evolved from his own brain for certain diseases: Quackem's Elixir for Coughs and Colds: invaluable in Bronchitis, &c. In many of these "chest mixtures" I am afraid there is opium, and the danger of administering these to old people suffering from bronchitis may be imagined. Verily, "fools step in where angels fear to tread." But druggists are not wholly to blame. In these days so many cheap manuals on self-doctoring and newspaper articles on medical subjects are published that the public are beginning to think they can diagnose their own diseases, and they simply apply to the druggist for the remedies recommended in these publications. As to the second set of causes, arising from the conduct of medical men themselves, much might be said, but there is one line of conduct which is becoming commoner every day—i.e., under-cutting one's neighbour in the matter of fees. This, of course, may arise from the terrible struggle for existence, but in many cases it is simply an underhand way of increasing a practice at the expense of a medical brother, as it is done in the case of well-to-do patients every day. What is the remedy for this kind of thing? A universal tariff of minimum charges should be established and every medical man should be bound not to charge less than these. Again, large numbers of medical men are let loose on the public every year who have no idea of medical etiquette or of their duty towards their neighbour, and who will filch a patient by any means, however unfair, and can see no harm in so doing. I think medical students should have special lectures on medical etiquette and their duties to their neighbour, and should give some guarantee before launching into practice that they will faithfully abide, so far as possible, by the rules of honour and probity laid down for their guidance. The status of the profession can also be ameliorated by raising the standard of general knowledge and culture before allowing students to begin their special medical studies. I have said nothing about the harm done